Steenstrup (J.), For. Mem. R.S. Mammuthjæger-Stationen ved Předmost. 8vo. *Kjöbenhavn* 1889. With two other Excerpts in 8vo. The Author.

Tejera (M.) Origen y Constitución Mecánica del Mundo. 8vo. *Barcelona* 1889. The Author.

Volante (A.) Eureka Areostatica ai pie' della Ferrea Corona. 4to. *Torino* 1888. The Author.

Walker (J. F.) Communications [extracted] from the Yorkshire Philosophical Society's Report, 1888. 8vo. The Author.

June 20, 1889.

Professor Sir G. GABRIEL STOKES, Bart., President, in the Chair.

Mr. John Aitken, Dr. E. Ballard, Mr. A. B. Basset, Mr. Horace T. Brown, Mr. Latimer Clark, Mr. Lazarus Fletcher, Mr. W. B. Hemsley, Dr. C. T. Hudson, Mr. E. B. Poulton, Professor W. J. Sollas, Mr. Herbert Tomlinson, and Professor G. F. Yeo were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The President announced to the Meeting that it had that afternoon been resolved by himself and the Council to address a letter to the Lord Mayor of London, expressing sympathy with his attempt to obtain some public recognition in this country of services rendered by M. Pasteur to science and humanity, and that the officers, with Sir James Paget, Sir Joseph Lister, Sir Henry Roscoe, and Professor Lankester, had been appointed to represent the Society at the meeting which the Lord Mayor had called for July 1st.

The following Papers were read:—

I. "On the Cavendish Experiment." By C. V. Boys, A.R.S.M., F.R.S., Assistant Professor of Physics at the Normal School of Science, South Kensington. Received May 29, 1889.

The Cavendish experiment for determining the constant of gravitation, from which the density of the earth may be calculated, is so well known that there is no occasion to describe it. This experiment,

devised by the Rev. John Mitchell, F.R.S., was first carried out by Cavendish,* and has been since performed by Reich,† Baily,‡ and Cornu and Baille,§ who have all followed very closely the arrangement of Cavendish.

Owing to the very small value of the constant of gravitation, all these experimentalists have aimed at increasing the sensibility as much as possible. With this object, a long beam carrying at its ends considerable masses has been suspended by a very long and very fine wire. The attracting masses have been made as large as possible, and they have been brought almost into contact with the sides of the long box in which the beam is suspended. Cornu, it is true, has reduced the dimensions of all the parts to about one-quarter of the original amount. His beam, an aluminium tube, is only half a metre long, and it carries at its ends masses of $\frac{1}{4}$ lb. each, instead of about 2 lb. as used by Cavendish. This reduction of the dimensions to about one-quarter of those used previously is considered by Cornu to be one of the advantages of his apparatus, because, as he says, if the period of oscillation is unchanged, then the sensibility is independent of the mass of the suspended balls, and is inversely as the linear dimensions. I do not quite follow this, because, as I shall show, if all the dimensions are increased or diminished together the sensibility will be unchanged. If only the length of the beam is altered and the positions of the large attracting masses, so that they remain opposite to and the same distance from the ends of the beam, then the sensibility is inversely as the length.

The other improvements introduced by Cornu are the use of mercury for the attracting masses which can be drawn from one pair of vessels to the other without coming near the apparatus, the use of a metal case connected with the earth to prevent electrical disturbances, and the electrical registration of the movements of the index on the scale which was placed 560 cm. from the mirror. The period of oscillation which has been used has varied between 398 seconds (Cornu) and 840 seconds (Cavendish). Cavendish found that with the very inconvenient period of 1800 seconds the balls knocked against the side of the case.

The difficulty that has been met with has been the perpetual shifting of the position of rest, due partly to the imperfect elasticity or fatigue of the torsion wires, and partly, as Cavendish proved experimentally, to the enormous effects of air currents set up by temperature differences in the box, which with large apparatus it is impossible to prevent. In every case the power of observing was in

* 'Phil. Trans.,' 1798, p. 469.

† 'Comptes Rendus,' 1837, p. 697.

‡ 'Phil. Mag.,' vol. 21, 1842, p. 111.

§ 'Comptes Rendus,' vol. 76, p. 954; vol. 86, pp. 571, 699, 1001.

excess of the constancy of the effect actually produced. The observations of Cornu are the only ones which are comparable in accuracy with other physical measurements, and these, as far as the few figures given enable one to judge, show a very remarkable agreement between values obtained for the same thing from time to time.

Soon after I had made and found the value of quartz fibres for producing a very small and constant torsion, I thought that it might be possible to apply them to the Cavendish apparatus with advantage. Professor Tyndall, in a letter to a neighbour written some months ago, expressed the conviction that it would be possible to make a much smaller apparatus in which the torsion should be produced by a quartz fibre. Last summer I began to prepare an instrument with a working beam five millimetres long, but other experimental work obliged me to put this on one side for a time. I have lately examined the theory of this instrument in some detail, and as I find that in many particulars there is an advantage in departing from the arrangement that has always been employed, I have lately prepared two pieces of apparatus, which on trial fully bear out the results of this inquiry.

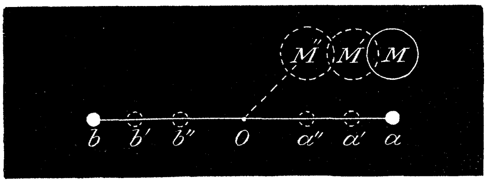
I shall, therefore, first give a short account of the principles that should be followed in the design of the Cavendish apparatus, and then describe the results which I have obtained up to the present time.

As I have already stated, the sensibility of the apparatus is, if the period of oscillation is always the same, independent of the linear dimensions of the apparatus. Thus, if there are two instruments in which all the dimensions of one are n times the corresponding dimensions of the other, then the moment of inertia of the beam and its appendages will be as $n^5 : 1$, and, therefore, the torsion also must be as $n^5 : 1$. The attracting masses, both fixed and movable, will be as $n^3 : 1$, and their distance apart as $n : 1$. Therefore, the attraction will be as n^6/n^2 or $n^4 : 1$, and this is acting on an arm n times as long in the large instrument as in the small, therefore the moment will be as $n^5 : 1$, that is, in the same proportion as the torsion, and so the angle of deflection is unchanged.

If, however, the length of the beam only is changed, and the attracting masses are moved until they are opposite to and a fixed distance from the ends of the beam, then the moment of inertia will be altered in the ratio $n^2 : 1$, while the corresponding moment will only change in the ratio of $n : 1$, and thus there is an advantage in reducing the length of the beam until one of two things happens, either it is difficult to find a sufficiently fine torsion thread that will safely carry the beam and produce the required period, and this, I believe, has up to the present time prevented the use of a beam less than half a metre in length, or else when the length becomes nearly equal to the diameter of the attracting balls, they then act with such an

increasing effect on the opposite suspended balls, so as to tend to deflect the beam in the opposite direction, that the balance of effect begins to fall short of that which would be due to the reduced dimensions if the opposite ball did not interfere. Fig. 1 will make the meaning more clear. *ab* is the beam of the ordinary apparatus with a ball at

FIG. 1.



each end. M is one of the attracting masses, and the other one occupies a symmetrical position on the opposite side of the centre O, but as the relations of each with the moving system are identical, it will be sufficient to consider only one.

As the beam is supposed to become shorter, the small balls will occupy successively the places $a'b'$ and $a''b''$, while the large mass M will take the corresponding places shown by the dotted circles at M′ and M″. When it has reached the position M″, at which the line joining its centre with O makes an angle of 45° with *ab*, the sensibility of the combination is still increasing, but not quite so fast as it would do if the attraction on the ball *b* did not partly counteract the attraction on the ball *a*. Should this position be chosen for the mass M, then the beam of the length $a''b''$ is not the best that can be used, if it is further shortened the sensibility will be still further increased, and will become a maximum when the beam has a length equal to half $a''b''$, that is, when the distance between the large balls is $2\sqrt{2}$ times the distance between the small ones. If the length of the beam is made successively equal to 1, 2, 3 . . . . . 10 tenths of the distance $a''b''$, then the corresponding deflections will be represented by the numbers in the following table:—

ab.	Deflection.
0·1	1·050
0·2	1·070
0·3	1·077
0·4	1·082
0·5	1·088
0·6	1·080
0·7	1·066
0·8	1·037
0·9	0·982
1·0	0·911

The unit deflection is that which would be produced if each large ball acted only on the small ball near it, and if the small balls occupied the positions $a''b''$.

If the position which is chosen for each attracting mass is nearer the plane of the beam than the transverse plane, that is, if the azimuth of the large masses is less than 45°, the best length of the beam will be more than half that which would bring the ends opposite the attracting masses.

It might be urged against this argument that a difficulty would arise in finding a torsion fibre that would give to a very short beam loaded with balls that it will safely carry a period as great as five or ten minutes, and until quartz fibres existed there would have been a difficulty in using a beam much less than a foot long, but it is now possible to hang a thing only half an inch long and weighing from 20 to 30 grains by a fibre not more than a foot in length, so as to have a period of five minutes. If the moment of inertia of the heaviest beam of a certain length that a fibre will safely carry is so small that the period is too rapid, then the defect can be remedied by reducing the weight, for then a finer fibre can be used, and since the torsion varies approximately as the square of the strength (not exactly because fine fibres carry heavier weights in proportion), the torsion will be reduced in a higher ratio, and so by making the suspended parts light enough, any slowness that may be required may be provided.

Practically, it is not convenient to use fibres much less than one ten-thousandth of an inch in diameter, and these have a torsion ten thousand times less than that of ordinary spun glass. A fibre one five-thousandth of an inch in diameter will carry a little over 30 grains.

Since with such small apparatus as I am now using it is easy to provide attracting masses which are very large in proportion to the length of the beam, while with large apparatus comparatively small masses must be made use of owing to the impossibility of dealing with balls of lead of great size, it is clear that much greater deflections can be produced with small than with large apparatus. For instance, to get the same effect in the same time from an instrument with a 6-foot beam that I get from one in which the beam is five-eighths of an inch long, and the attracting balls are 2 inches in diameter, it would be necessary to provide and deal with a pair of balls each 25 feet in diameter and weighing 730 tons instead of about $1\frac{3}{4}$ lb. apiece. There is the further advantage in small apparatus that if for any reason the greatest possible effect is desired, attracting balls of gold would not be entirely unattainable, while such small masses as two piles of sovereigns could be used where qualitative effects only were to be shown. Owing to its strongly magnetic qualities, platinum is unsuited for experiments of this kind.

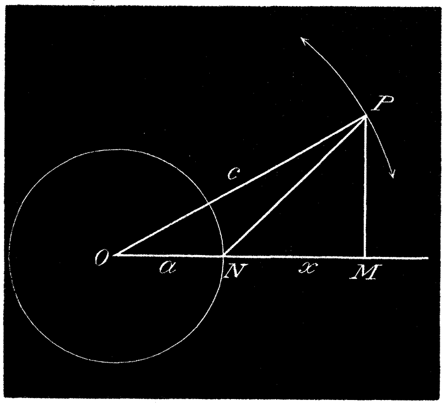
By far the greatest advantage that is met with in small apparatus is the perfect uniformity of temperature which is easily obtained; whereas, with apparatus of large size, this alone makes really accurate work next to impossible. The construction to which this inquiry has led me, and which will be described later, is especially suitable for maintaining a uniform temperature in that part of the instrument in which the beam and mirror are suspended.

With such small beams as I am now using it is much more convenient to replace the long thin box generally employed to protect the beam from disturbance by a vertical tube of circular section, in which the beam with its mirror can revolve freely. This has the further advantage that if the beam is hung centrally, the attraction of the tube produces no effect, and the troublesome and approximate calculations which have been necessary to find the effect of the box are no longer required. The attracting weights, which must be outside the tube, must be made to take alternately positions on the two sides of the beam, so as to deflect it first in one direction and then in the other. For this purpose they are most conveniently fastened to the inside of a larger metal tube which can be made to revolve on an axis coincident with the axis of the smaller tube. There are obviously two planes, one containing and one at right angles to the beam, in which the centres of the attracting balls will lie when they produce no deflection. At some intermediate position the deflection will be a maximum. Now it is a matter of some importance to choose this maximum position for the attracting masses, because, in showing the experiment to an audience, the largest effect should be obtained that the instrument is capable of producing; while in exact measures of the constant of gravitation this position has the further advantage that the only measurement which there is any difficulty in making, viz., the angle between the line joining the large masses and the line joining the small, which may be called the azimuth of the instrument, becomes of little consequence under these circumstances. In the ordinary arrangement the slightest uncertainty in this angle will produce a relatively large uncertainty in the result. I have already stated that if an angle of 45° is chosen, the distance between the centres of the large balls should be $2\sqrt{2}$ times the length of the beam, and the converse of course is true. As it would not be possible at this distance to employ attracting balls with a diameter much more than one and a half times the length of the beam, and as balls much larger than this are just as easily made and used, it will be well to find out what will be the position for maximum deflection when the centres of the attracting balls are any distance apart.

In the case already considered the problem gives rise to equations of too high an order to be readily solved, and so in the particular case referred to the result was obtained by arithmetical means. If the

effect in the nearer ball only is considered, then it is easy to find the best position for any distance of the attracting mass from the axis of motion. Let P (fig. 2) be the centre of the attracting ball, N that of

FIG. 2.



the nearer attracted ball, O the axis of motion, c and a the distances of P and N from O, and x the distance from N of the foot of the perpendicular from P on ON produced. Then the moment of N about O will be greatest when

$$x^2 + \frac{3a^2 + c^2}{a} x = 2(c^2 - a^2),$$

or what comes to the same thing when

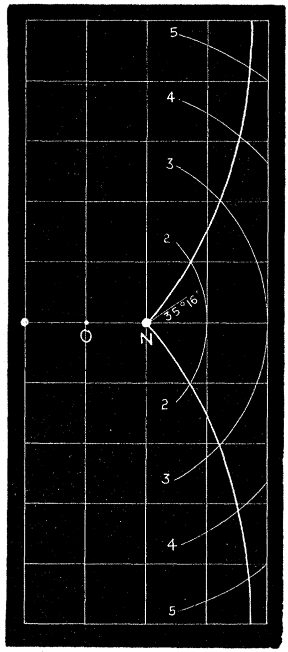
$$\cos^2\theta + \frac{c^2 + a^2}{ca} \cos\theta = 3.$$

The solutions of these equations are given in the following table:—

c/a.	θ		x/a.
1	0°	0′	0
2	27	45	0·77
3	42	16	1·22
4	51	19	1·50
5	58	20	1·62
6	63	15	1·70
7	66	44	1·76
8	69	23	1·82
9	71	17	1·88
10	72	50	1·95
∞	90	0	2·00

These figures are represented by the curve in fig. 3, which shows the best position for an attracting mass at any distance from the

FIG. 3.



axis O. The inclination of this with the line ON at the point N is 35° 16′, or an angle of which the tangent is equal to $1/\sqrt{2}$. This curve also shows the best position from which a source of light at any distance from O would most brightly illuminate a small surface at N lying along ON.

If now an attracting ball is placed in a position of maximum effect with its centre on this line it will act on the further suspended ball, tending to deflect the beam in the opposite direction, and this will become more marked as the distance between the centres of the attracting balls increases, and so the increased effect which would be due to a greater attracting ball may be largely compensated by the increased action on the remote end of the beam. The azimuth at which the maximum effect is produced is also changed.

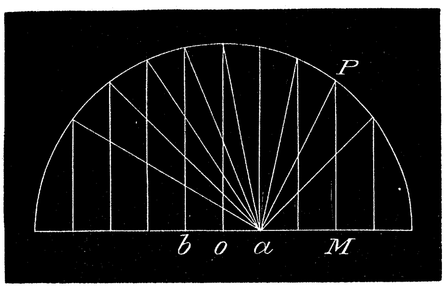
I have practically overcome this difficulty by arranging the two sides of the apparatus at different levels. Each large ball is at or near the same level as the neighbouring small ball, but one pair is removed from the level of the other by about the diameter of the large balls which in the apparatus which I have now the honour to

submit to the Society is nearly five times as great as the distance *in plan* between the two small balls.

In order to realise more fully the effect of a variety of arrangements, I have, for my own satisfaction, calculated the values of the deflecting forces in an instrument in which the distance between the centres of the attracting balls is five times the length of the beam, for every azimuth and for differences of levels of 0, 1, 2, 3, 4, and 5 times the length of the beam.

This calculation is very much facilitated by the property of the circle illustrated in fig. 4. If the diameter is divided into any number of equal parts, and perpendiculars drawn to cut the circle, then the squares of lines drawn from any one of the points on the diameter to all the intersections (including the two ends of the diameter) are in arithmetical progression, and the common difference is equal to twice

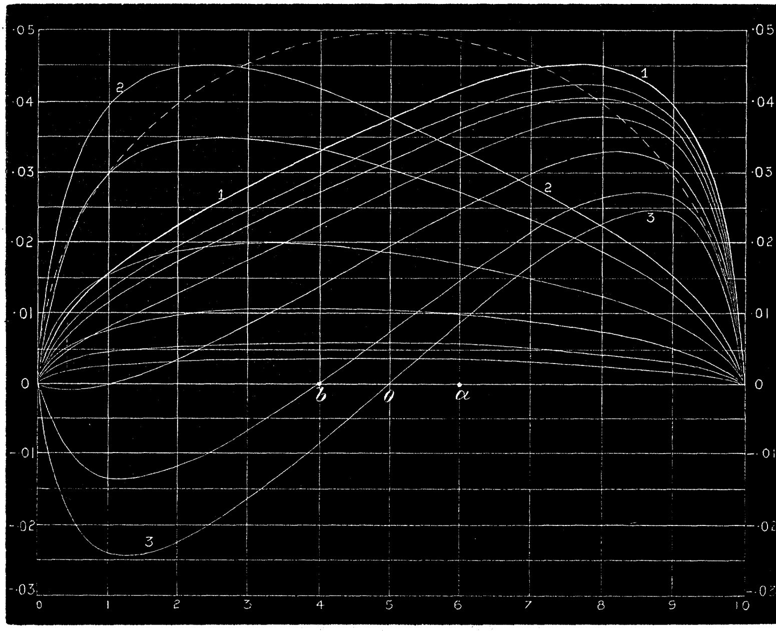
FIG. 4.



the number of parts included between that point and the centre. If the diameter is divided into ten parts, a and b are the positions of the ends of the beam, and the semicircle is the path of the centre of the large mass. When this is at any position P the resolved force at a is equal to $\mathrm{PM}/\mathrm{P}a^3$. Now all the quantities PM^2 and $\mathrm{P}a^2$ are small whole numbers, and the squares of the true distances of P from a when a is at different levels are small whole numbers also, so that all the logarithms can be found on the first four pages of Chambers's tables. It is for this reason that it is most convenient to represent the result of the calculation on a diagram in which the abscissæ are the projections of the centre of the attracting mass on a plane passing through the centres of the small balls.

In fig. 5 the dotted circle represents the possible positions of the centre of the attracting mass, and a, b the small balls. The heavy Curve 1 shows the value of the moment due to the ball a alone. The reversed Curve 2 in the same way shows the moment of the ball b in the opposite direction when that ball is at the same level as a. The Curve 3 is the difference between these two, and from this actual resultant moments may be found. The maximum of this curve is in

FIG. 5.



a slightly different position from that of the Curve 1, and its greatest value is only just over half that of the first curve, which shows that the sensibility can be nearly doubled by lowering the ball *b* until it is too far down to be appreciably attracted by the ball which is on a level with *a*. The five curves below 2 show the effect of lowering *b* until it is 1, 2, 3, 4 and 5 times the length of *ab* below the general level, and the five curves between 1 and 3 in the same way represent the values of the balance in favour of *a* when *b* is at these different levels. The curves when drawn are instructive in that they show both the relative advantages of the various differences of levels, and from the curvature at the maximum positions the practical importance of correctly determining the azimuth. By reference to the number on the vertical scale it is also easy to directly compare the sensibility of the apparatus with any of the arrangements which have been in use. For instance, calling in every case the half length of the beam unity and the dimensions of the other parts by numbers in proportion, we have in the apparatus of Cavendish—

Equivalent distance between centres of large and small balls (8·95 inches)	0·249
Diameter of large balls (12 inches)	0·333
Comparative value of deflecting force $0{\cdot}333^3/0{\cdot}249^2$	0·596

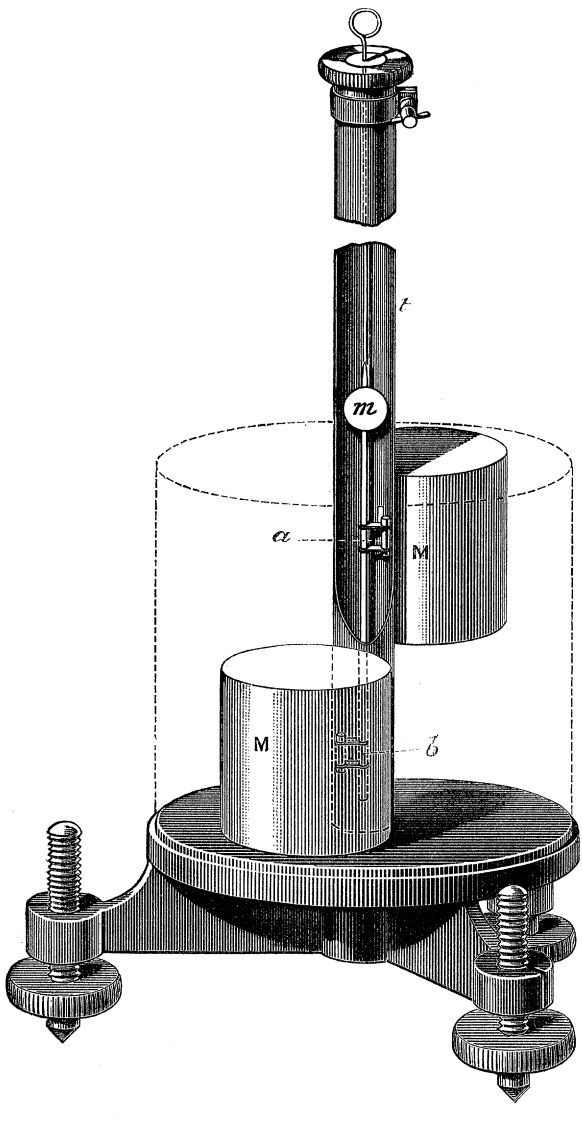
In my apparatus it is simply necessary to multiply the cube of the diameter of the large balls by the ordinate of the curve, to find on the same scale the value of the deflection. This requires that the large balls should be made of material of the same density in the two cases, and that the periods should be the same. Now the diameter of the large ball in the new apparatus is 6·4 times the length of the beam, and so the comparative value of the deflecting force is $0{\cdot}0425 \times 6{\cdot}4^3 = 11{\cdot}1$, a figure which is 18·7 times as great as the figure found for the apparatus of Cavendish. If then the large balls have the same density as those used by Cavendish and the period of oscillation is the same, the angular deflection will be 18·7 times as great.

Having now found that with apparatus no bigger than an ordinary galvanometer it should be possible to make an instrument far more sensitive than the large apparatus in use heretofore, it is necessary to show that in practice such a piece of apparatus will practically work, and that it is not liable to be disturbed by the causes which in large apparatus have been found to give so much trouble.

I have made two instruments of which I shall only describe the second, as that is better than the first both in design and in its behaviour.

The construction of this is made clear by fig. 6. To a brass base provided with levelling screws is fixed the vertical brass tube, t, which forms the chamber in which the small masses a, b are suspended by a quartz fibre from the pin at the upper end. These little masses are cylinders of pure lead 11·3 mm. long and 3 mm. in diameter, and the vertical distance between their centres is 50·8 mm. They are held by light brass arms to a very light taper tube of glass, so that their axes are 6·5 mm. from the axis of motion. The mirror m, which is 12·7 mm. in diameter, plane and of unusual accuracy, is fastened to the upper end of the glass tube by the smallest quantity of shellac varnish. Both the mirror and the plate-glass window which covers an opening in the tube were examined and afterwards fixed with the refracting edge of each horizontal, so that the slight but very evident want of parallelism between their faces should not interfere with the definition of the divisions of the scale. The large masses M, M are two cylinders of lead 50·8 mm. in diameter, and of the same length. They are fastened by screws to the inside of a brass tube, the outline of which is dotted in the figure, which rests on the turned shoulder of the base so that it may be twisted without shake through any angle. Stops (not shown in the figure) are screwed to the base, so that the actual angle turned through shall be that which produces the maximum deflection. A brass lid made in two halves covers in the outer tube and serves to maintain a very perfect uniformity of temperature in the inner tube. Neither the masses M, M, nor the lid touch the inner tube. The period of oscillation is 80 seconds.

Fig. 6.



With this apparatus placed in an ordinary room with draughts of air of different temperatures and with a lamp and scale such as are used with a galvanometer, the effect of the attraction can easily be shown to a few, or, with a lime-light, to an audience. To obtain this

result with apparatus of the ordinary construction and usual size is next to impossible, on account chiefly of the great disturbing effect of air currents set up by difference of temperature in the case. The extreme portability of the new instrument is a further advantage, as is evident when the enormous weight and size of the attracting masses in the ordinary apparatus are considered.

However, this result is only one of the objects of the inquiry which I have now the honour to submit to the Royal Society. The other object which I had in view was to find whether the small apparatus, besides being more sensitive than that hitherto employed, would also be more free from disturbances and so give more consistent results. With this object I have placed the apparatus in a long narrow vault under the private road between the Museum and the Science Schools. This is not a good place for experiments of this kind, for when a cab passes overhead the trembling is so great that loose things visibly move; however, it is the only place at my disposal that is in any degree suitable. A large drain pipe filled with gravel and cement and covered by a slab of stone forms a fairly good table. The scale is made by etching millimetre divisions on a strip of clear plate glass 80 cm. long. This is secured at the other end of the vault at a distance of 1053·8 cm. from the mirror of the instrument. A telescope 132 cm. long and with an object-glass 5·08 cm. in diameter rests on **V**'s clamped to the wall, with its object-glass 360 cm. from the mirror. Thus any disturbance that the observer might produce if nearer is avoided and at the same time the field of view comprises 100 divisions. While the observer is sitting at the telescope he can by pulling a string move an albo-carbon light mounted on a carriage so as to illuminate any part of the scale that may happen to be in the field of the telescope. The white and steady flame forms a brilliant background on which the divisions appear in black. The accuracy of the mirror is such that the millimetre divisions are clearly defined, and the position of the cross-wire (a quartz fibre) can be read accurately to one-tenth of a division. This corresponds to a movement of the mirror of almost exactly one second of arc.

The mode of observation is as follows: When all is quiet with the large masses in one extreme position, the position of rest is observed and a mark placed on the scale. The masses are moved to one side for a time and then replaced which sets up an oscillation. The reading of every elongation and the time of every transit of the mark are observed until the amplitude is reduced to three or four centimetres. The masses are then moved to the other extreme position and the elongations and transits observed again, and this is repeated as often as necessary.

On the evening of Saturday, May 18th, six sets of readings were

taken, but during the observations there was an almost continuous tramp of art students above, producing a perceptible tremor, besides which two vehicles passed, and coals were twice shovelled in the coal cellar, which is separated from the vault in which the observations were made by only a four and a half inch brick wall. The result of all this was a nearly perpetual tremor, which produced a rapid oscillation of the scale on the cross-wire, extending over a little more than 1 mm. This increased the difficulty of taking the readings, but to what extent it introduced error I shall not be able to tell until I can make observations in a proper place.

In spite of these disturbances, the agreement between the deflections deduced from the several sets of observations and between the periods is far greater than I had hoped to obtain, even under the most favourable conditions. In order to show how well the instrument behaved, I have copied from my note-book the whole series of figures of one set, which sufficiently explain themselves.

Elongation.	Amplitude.	Decrement.	True position of rest.	Time of transit of 36·09.	Correction for transit of true position of rest.	True half period.
				h. m. s.		
15·05						
53·20	38·15	0·805	36·18	9 8 25·0	+0·08	
22·48	30·72	0·808	36·20	9 45·5	−0·18	80·2
47·28	24·80	0·807	36·21	11 5·3	+0·24	80·2
27·28	20·00	0·807	36·20	12 25·8	−0·28	80·0
43·40	16·12	0·805	36·22	13 45·0	+0·41	79·9
30·42	12·98	0·806	36·21	15 6·0	−0·47	80·1
40·88	10·46	0·802	36·22	16 25·0	+0·63	80·1
32·50	8·38	0·808	36·24	17 46·0	−0·91	79·5
39·27	6·77	0·808	36·24	19 4·5	+1·13	80·5
33·80	5·47	0·814	36·26	20 27·0	−1·58	79·8
38·25	4·45		36·26	21 44·0	+1·94	80·5
		0·8066				80·08

It will be noticed that the true position of rest is slightly rising in value, and this rise was found to continue at the rate of 0·36 cm. an hour during the whole course of the experiment, and to be the same when the large masses were in the positive or negative position. The motion was perfectly uniform, and in no way interfered with the accuracy of the experiments. It was due, I believe, to the shellac fastening of the fibre, for I find that immediately after a fibre has been attached this movement is very noticeable, but after a few days it almost entirely ceases; it is, moreover, chiefly evident when the

fibre is loaded very heavily. At the time that the experiment was made the instrument had only been set up a few hours.

The mean decrement of three positive sets was 0·8011, and of three negative sets, 0·8035. The observed mean period of three positive sets was 79·98, and of three negative sets, 80·03 seconds, from both of which 0·20 must be deducted as the time correction for damping.

The deflections obtained from the six sets of observations taken in groups of three, so as to take into account the effect of the slow change of the position of rest, were as follows:—

From sets 1, 2, and 3	17·66 ± 0·01
,, 2, 3, and 4	17·65 ± 0·02
,, 3, 4, and 5	17·65 ± 0·02
,, 4, 5, and 6	17·65 ± 0·02

An examination of these figures shows that the deflection is known with an accuracy of about one part in two thousand, while the period is known to the four thousandth part of the whole. As a matter of fact the discrepancies are not more than may be due to an uncertainty in some of the observations of half a millimetre or less, a quantity which, under the circumstances, is hardly to be avoided.

The result of these experiments is complete and satisfactory. As a lecture experiment the attraction between small masses can be easily and certainly shown, even though the resolved force causing motion is, as in the present instance, no more than the one two hundred-thousandth of a dyne (less than one ten-millionth of the weight of a grain), and this is possible with the comparatively short half period of eighty seconds. Had it been necessary to make use of such half periods as three to fifteen minutes which have been employed hitherto, then, even though a considerable deflection were produced, this could hardly be considered a lecture experiment.

The very remarkable agreement between successive deflections and periods shows that an absolute measure made with apparatus designed for the purpose, but on the lines laid down above, is likely to lead to results of far greater accuracy than any that have been obtained. For instance, in the original experiment of Cavendish there seems to have been an irregularity in the position of rest of one-tenth of the deflection obtained, while the period showed discrepancies of five to fifteen seconds in seven minutes. The experiments of Baily made in the most elaborate manner were more consistent, but Cornu was the first to obtain from the Cavendish apparatus results having a precision in any way comparable to that of other physical measurements. The three papers, published by him in the 'Comptes Rendus,' of 1878, referred to above, contain a very complete solution of some of the problems to which the investigation has given rise. The agreement between the successive values, decrement, and period is much the same that I have

obtained, nevertheless the means of the summer and of the winter observations differ by about 1 per cent.

I have not referred to the various methods of determining the constant of gravitation in which a balance, whether with the usual horizontal beam, or with a vertical beam on the metronome principle, is employed. They are essentially the same as the Cavendish method, except that there is introduced the friction of the knife-edges and the unknown disturbances due to particles of dust at these points, and to buoyancy, without, in my opinion, any compensating advantage. However, it would appear that if the experiment is to be made with a balance, the considerations which I have advanced in this paper would point to the advantage of making the apparatus small, so that attracting masses of greater proportionate size may be employed, and the disturbance due to convection reduced.

It is my intention, if I can obtain a proper place in which to make the observations, to prepare an apparatus specially suitable for absolute determinations. The scale will have to be increased, so that the dimensions may be determined to a ten-thousandth part at least. Both pairs of masses should, I think, be suspended by fibres or by wires, so that the distance of their centres from the axis may be accurately measured, and so that in the case of the little masses the moment of inertia of the beam, mirror, &c., may be found by alternately measuring the period with and without the masses attached. The unbalanced attractions between the beam, &c., and the large masses, and between the little masses and anything unsymmetrical about the support of the large masses, will probably be more accurately determined experimentally by observing the deflections when the large and the small masses are in turn removed, than by calculation.

If anything is to be gained by swinging the small masses in a good Sprengel vacuum, the difficulty will not be so great with apparatus made on the scale I have in view, *i.e.*, with a beam about 5 cm. long, as it would with large apparatus. With a view to reduce the considerable decrement, I did try to maintain such a vacuum in the first instrument, in which a beam 1·2 cm. long was suspended by a fibre so fine as to give a complete period of five minutes, but though the pump would click violently for a day perhaps, leakage always took place before long, and so no satisfactory results were obtained.

With an apparatus such as I have described, but arranged to have a complete period of six minutes, it will be possible to read the scale with an accuracy of one ten-thousandth of the deflection, and to determine the time of vibration with an accuracy about twice as great.

FIG. 1.

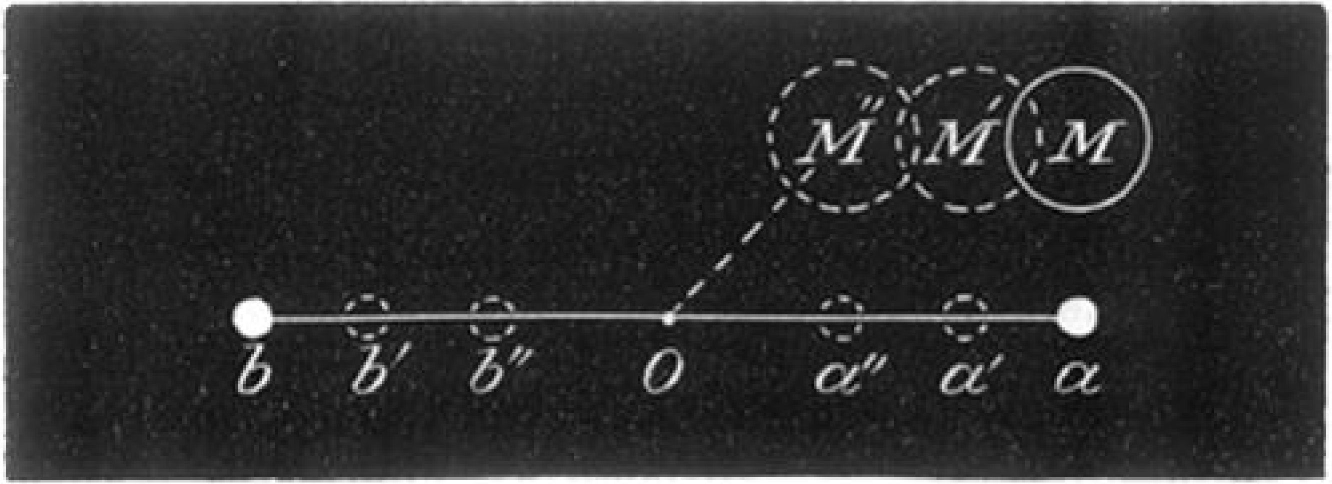


FIG. 2.

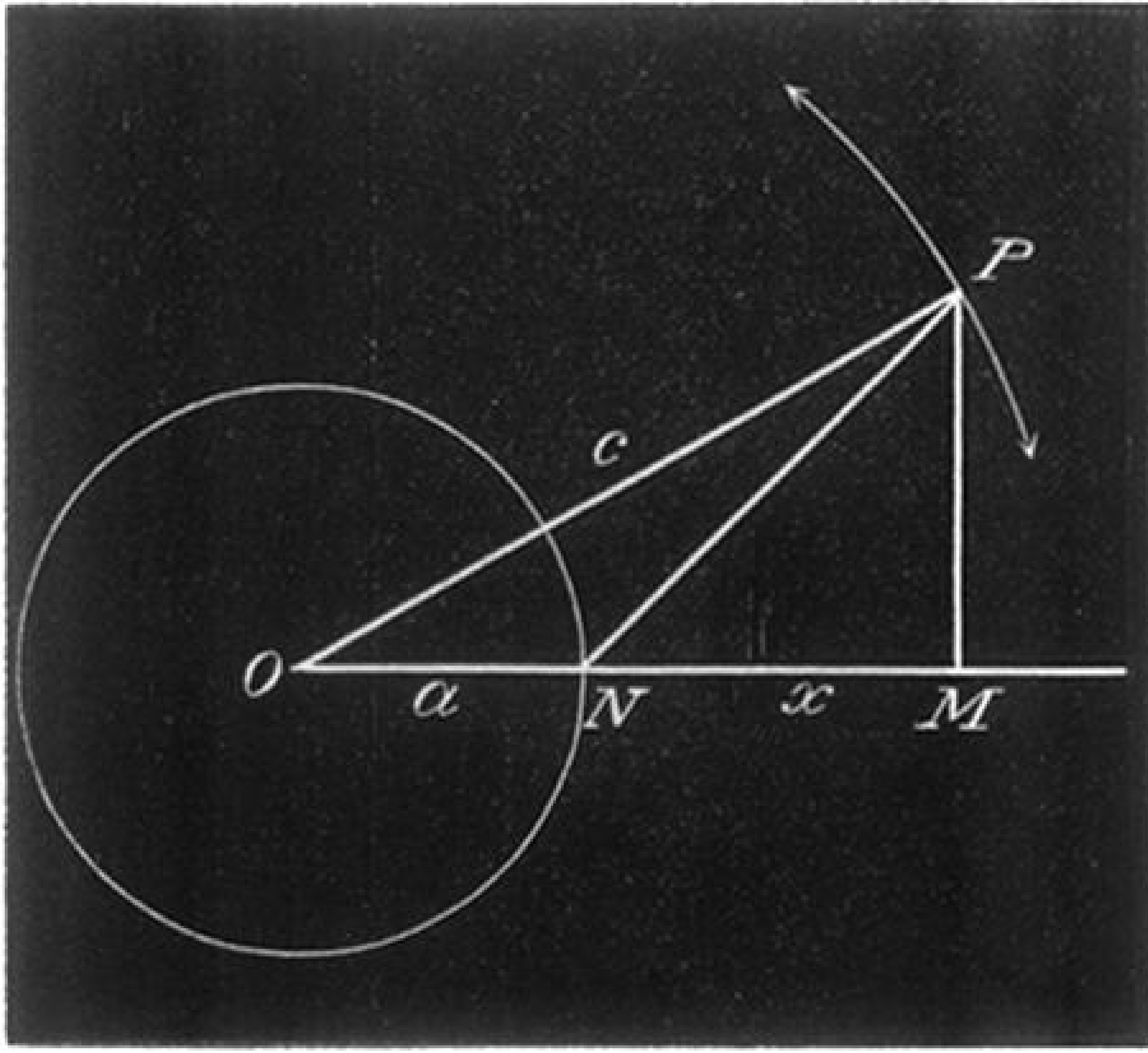


FIG. 3.

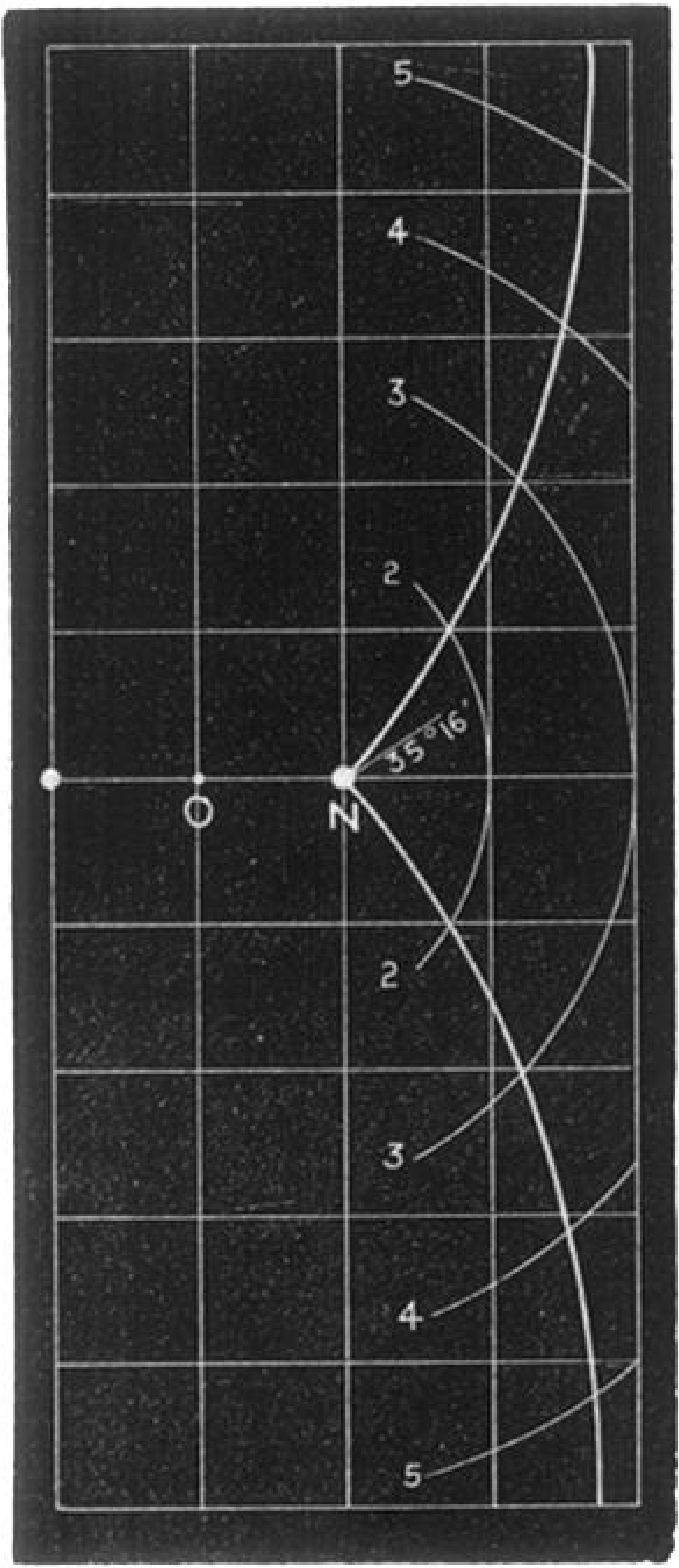


FIG. 4.

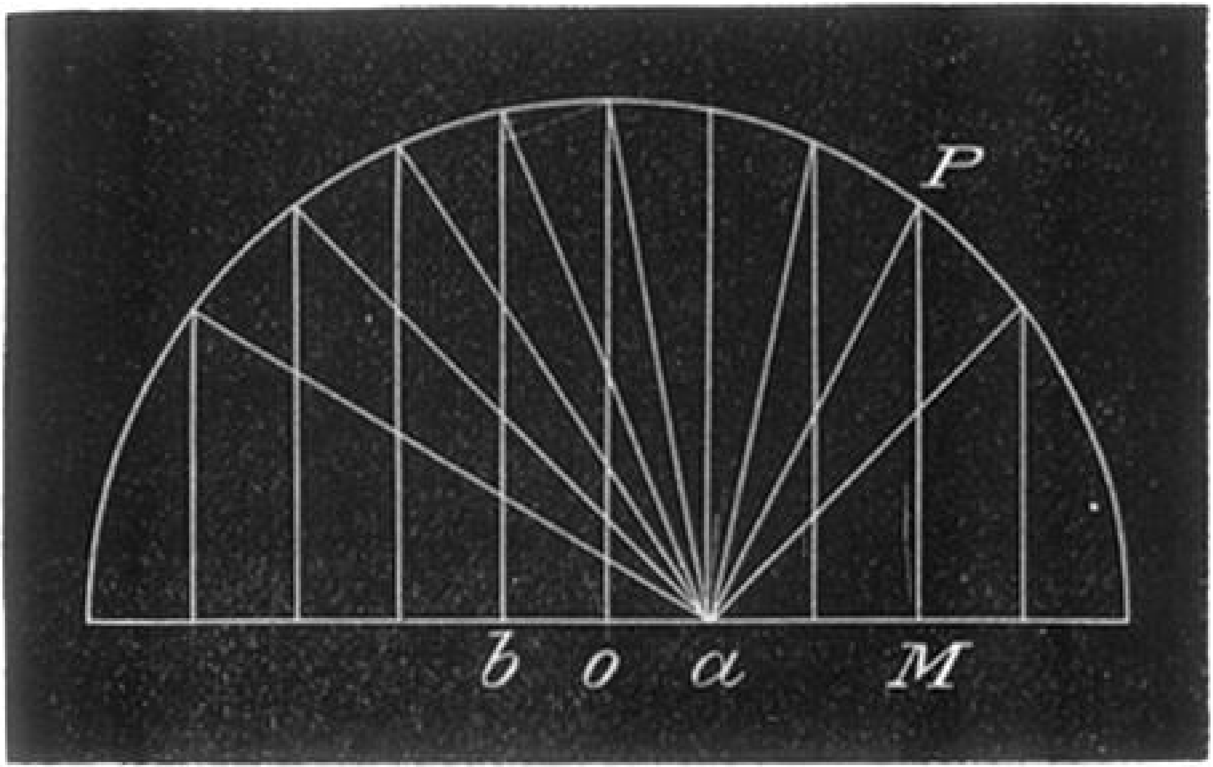


Fig. 5.

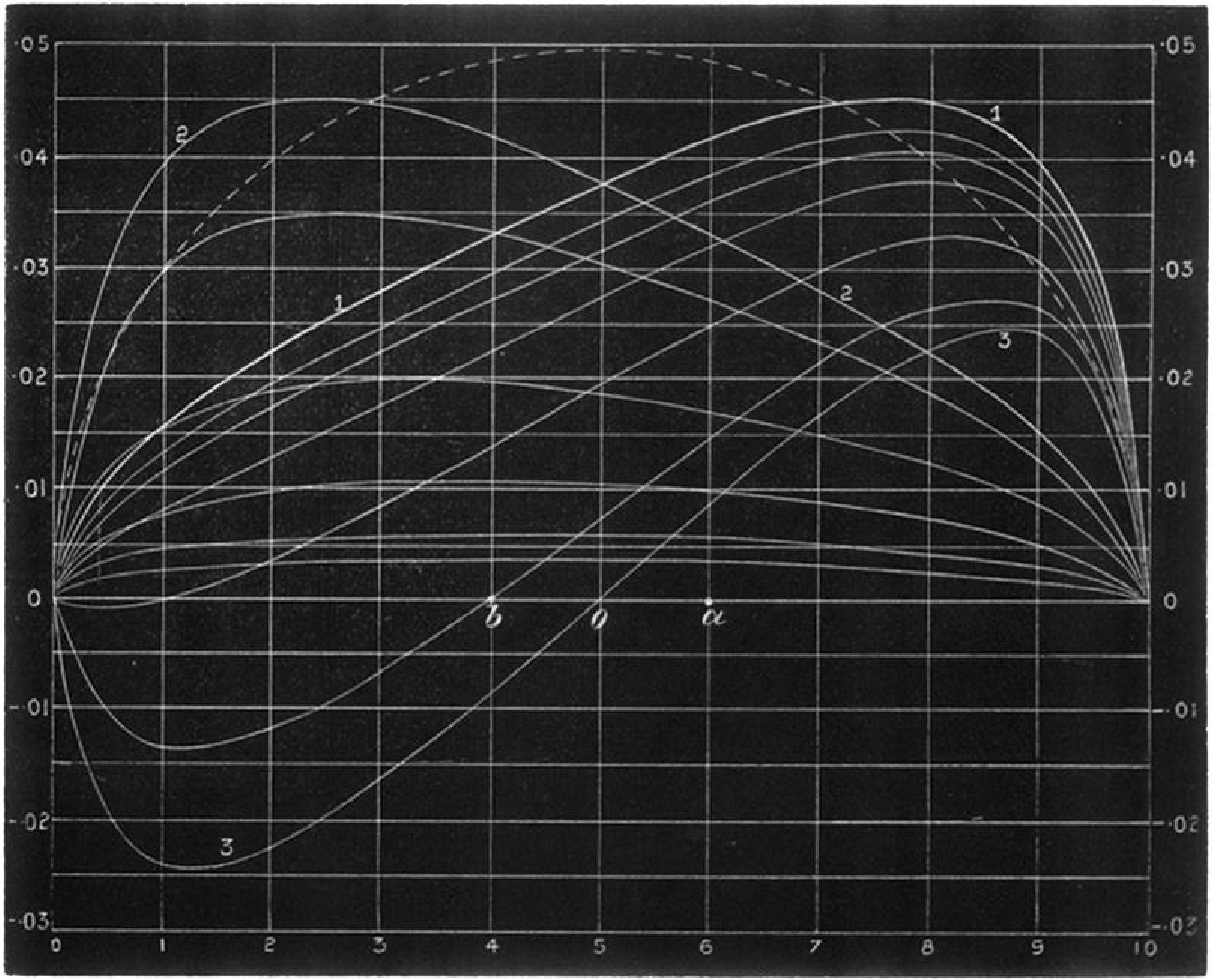


FIG. 6.